LETTERS TO THE EDITOR.

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The Complex Nature of Thorium.

With regard to several letters on thorium and its complex nature that appeared in Nature of March 24 and 31, April 7 and 14, and in which my name is mentioned, I take the liberty of adding a few remarks, having had ten years' experience in working with thorium.

In 1897, at a meeting of the British Association in Toronto (Canada), I read a paper in which I pointed out that spectrum evidence proves the complex nature of thorium.

In 1898 (Chem. Soc. *Trans.*, p. 953) I isolated from some thorium fractions an earth with an atomic weight of 225·8 (tetrad). Knowing the difficulties of the separation of rare earths (I have been engaged in this kind of work since 1878), and not wishing to publish a premature conclusion, I did not declare this to be a novel constituent of thorium, but said that foreign earths were present, in spite of the fact that the reaction used ought to have separated them.

In 1901 I published another short paper (*Proc.* Chem. Soc., March 21, 1901, pp. 67–68), in which I said that "my experiments may be regarded as proving the complex nature of thorium." Thorium was split up into Thα and Thβ. With Thβ I obtained so low an atomic weight as $R^{iv} = 220$. The fractions Thα gave by the analysis of the oxalate, though it was prepared by pouring the thorium salt solution into an excess of oxalic acid, in order to avoid the formation of a basic salt, the high atomic weight $R^{iv} = 236\cdot3$. But I stated expressly, and I feel obliged to repeat it, that these fractions show a great tendency to form basic salts. Assuming these to be normal, a higher atomic weight than the true one is obtained. This is true especially in regard to the oxalate.

The splitting up of thorium into Thα and Thβ was, of course, not so sensational an event as the announcement from America of the splitting up of thorium into "carolinium" and "berzelium."   Bohuslav Brauner.

Bohemian University, Prague, April 18.

Radio-activity and the Law of Conservation of Mass.

Mr. Soddy in the Wilde lecture on the "Evolution of Matter as Revealed by the Radio-active Elements" (*Proc* Manchester Phil. Soc., vol. xlviii., part ii., p. 29) gives two methods of deducing the average life of a radium atom. The results become concordant if we assume that the complete disintegration of an atom of radium involves the emission of four α particles. Now the atomic mass of radium is 225, and that of an α particle about 2; the question therefore arises as to what has become of the rest of the mass.

There appear to be three possible answers to this question. In the first place Mr. Soddy's estimate may be wrong by a factor of ten, although it is hardly likely that the data are uncertain to this extent; secondly, the various stages of the disintegration may involve the liberation of non-radio-active by-products which would necessarily be incapable of detection by the methods of investigation employed; and, finally, there may be a decrease in the total mass of the system owing to the decrease in the velocities of some of the constituent electrons.

A priori the second hypothesis appears to have the balance of probability in its favour, as it agrees best with our present ideas; but I think the third solution should not be dismissed too hastily. In discussing the matter recently with Mr. G. A. Schott, I found that he also had been led to consider the tenability of this view in connection with some theoretical work on the structure of the atom which will probably soon be published.   O. W. Richardson.

Trinity College, Cambridge, April 19.

The Atomic Weight of Radium.

In the *Philosophical Magazine* for April, 1903, Runge and Precht work out the atomic weight of radium from its spectrum to be 258, instead of the 225 found by Madame Curie. I should like to point out that the spectrum data of radium support the value given by Madame Curie, if handled according to Section v. of "The Cause of the Structure of Spectra" (*Phil. Mag.*, September, 1901). There is an important question of principle involved in the distinction between the two methods of using the spectrum of an element for determining its place in the periodic classification.

The method adopted by Runge and Precht is founded on a slight alteration of the approximate empirical law discovered by Rydberg, that, when similar elements have corresponding pairs or triplets of lines in their spectra, the difference of the frequencies of vibration of the two lines in a pair belonging to any one element is proportional to the square of its atomic weight. Runge and Precht alter this law by substituting the words "some power of its atomic weight" for "the square of its atomic weight." Thus in the alkaline earth family they give the power as 1/0·5997. The frequency differences of corresponding pairs of lines in Mg, Ca, Sr and Ba, namely, 91·7, 223, 801 and 1691 being denoted by x, the logarithms of the atomic weights M of these elements are given by the formula $\log M = 0\cdot2005 + 0\cdot5997 \log x$. As x for radium is 4858·5, the value 258 is found by Runge and Precht for the atomic weight of radium. But the frequency differences are connected with one another by numerical relations, and not directly by their atomic weights.

The clearest instance of this purely numerical relationship is shown by the spectra of Zn, Cd, and Hg. A characteristic set of corresponding frequency differences for these elements is 386·4 for Zn, 1159·4 for Cd, and 4633·3 for Hg. The number for Zn multiplied by 3 gives 1159 2, which is indistinguishable from the value for Cd, and 12 times the number for Zn gives 4636 8, which is within 1 part in 1000 of the value for Hg.

In the paper mentioned I have shown how this numerical series, 1, 3, 12, appears elsewhere in the frequency differences for other families of elements, with a fourth member, namely, 28. Now these numbers are the first four terms of the series the general term of which is $1 - 3n/2 + 7n^2/2$. This series, and not atomic weights, controls the relations between the frequency differences for the spectra of allied elements.

Perhaps the most striking evidence in support of this assertion is afforded by some characteristic frequency differences discovered by Runge and Paschen in the complicated spectra of O, S, and Se. These are for O 3·7 and 2·08, for S 18·15 and 11·13, and for Se 103·7 and 44·07. Of these six numbers four are the first four terms of the series the general term of which is $3\cdot7(1 - 3n/2 + 7n^2/2)$, namely, 3·7, 11·1, 44·4, and 103·6. The numerical law applies, then, to the non-metals as well as to the metals. The distinction between a purely numerical and an atomic weight relation between the frequency differences of allied spectra is fundamental, the one implying a kinematical, the other a dynamical, origin for the structure of spectra.

As to the bearing of these considerations on the determination of the atomic weight of radium from its spectrum we can represent the differences given above for the alkaline earth elements by the formula $63\cdot8(1, 3, 12, 26) + 31\cdot6$, in which 26 for Ba takes the place of 28 in the standard series. There are other instances in which a serial number like 12 or 28 is reduced by 2 or 4, a phenomenon probably of kinematic origin. For the next three elements of this family with higher atomic weights than Ba, the frequency differences corresponding to those just given should be the values of $63\cdot8(1 - 3n/2 + 7n^2/2) + 31\cdot6$, given when $n = 4$, 5 and 6. The coefficients of 63·8 are 51, 81 and 118. Now the 4858·5 of Runge and Precht for radium is $63\cdot8 \times 75\cdot7 + 31\cdot6$. Thus the 75·7 of radium corresponds to the 81 of the regular series in the same way that the 26 of Ba does to the 28 of the regular series. So the numerical law for the frequency differences places radium two main rows lower than Ba in the table of elements, and gives to it, therefore, an atomic weight exceeding that of Ba, namely,

137, by about 90. The spectrum evidence confirms then the determination 225 made by Madame Curie.

As quite a number of investigators are working at relations between spectrum data and atomic weight, it seems important to make generally known the fact that mathematical series, like that of Balmer's law or that given above, are the main feature in the laws of spectra. The approximate law of Rydberg arises from the fact that the atomic weights of the elements form a series. Certain relations between this series and the series belonging to the spectra of the natural families are probably the cause of Rydberg's approximate law, which is not suitable for the extrapolation attempted by Runge and Precht with their modified form of it, unless other better means of estimating the atomic weight are lacking. All that we are warranted in saying at present is that the atomic weights of some of the elements in a family are nearly proportional to some power of $A + B(1 - 3n/2 + 7n^2/2)$, where n has positive integral values, and A and B are parameters characteristic of the family. WILLIAM SUTHERLAND.

Melbourne, March.

Graphic Methods in an Educational Course in Mechanics.

I SHOULD be glad if I could, through the columns of NATURE, elicit opinions from those who have taught mechanics from the beginning as to the advisability of either omitting graphical methods altogether from an educational course in mechanics or of introducing them at a very late stage.

By graphical methods I mean those methods which depend entirely on the use of mathematical instruments of precision, and from which calculation is absent. I do not refer to the plotting of curves from results obtained analytically, or to such simple graphical considerations as enable one to draw (freehand) a useful working figure.

I myself, after many years of teaching, have come to the conclusion that until the principles of statics and dynamics have been thoroughly grasped, it is better to keep graphical methods out of sight altogether.

My contentions are as follows :—

(1) *Analytical methods give a grasp of the principles of statics, while graphical methods disguise them.* When a body is at rest and in equilibrium, the obvious facts are that it does not move in this direction or in that, and does not rotate. Now the idea of a *resolute* as the effective component of a force in any direction is one readily grasped, and the analytical statement that " the resolutes in any direction balance one another " brings vividly before the mind the equilibrium as regards *translation*. Any experiment made suggests this balancing of resolutes. But the closing of a polygon of forces, on the contrary, does not suggest, with anything like the same degree of vividness, that there is no translation. In fact, the closed polygon of forces, representing as it does a couple, rather suggests that there *is* rotation. An experiment with a body on an inclined plane, for example, suggests a balance of resolutes and does not suggest a triangle of forces.

Again, as regards *rotation.* The analytical method of the " balancing of moments " brings clearly before the mind the fact that the body does not rotate. I am sure that most people will agree with me when I say that the corresponding graphical proposition, that " the funicular polygon closes," will not suggest non-rotation to any ordinary learner.

(2) *Analytical methods must be mastered in any case.* In any educational course, it is important that the learner shall have to rely on as few principles as possible. Now when he has mastered the principles of " resolution " and " taking moments," he can be led to attack any useful problem in statics without further theory.

But he may master the " polygon of forces " and the " funicular polygon," and yet find himself totally unable to deal with machines and with other constantly occurring cases of equilibrium. He will find that, while he can obtain by graphical methods the resultant of a system of

forces if these be parallel, he will probably fail if the forces be not parallel (and non-concurrent), owing to the difficulty of getting his diagram on to a given sheet of paper. In fact, analytical methods must be mastered, while graphical methods, however convenient in certain cases, need not be mastered save for special professional purposes. If, then, there be not time for both, it is the latter that should be sacrificed. A student well trained in analytical methods can always pick up graphical methods rapidly when he needs them for special work.

(3) *Analytical methods connect statics with dynamics.* I do not think that this contention will be disputed. Regarded analytically, statics are a part of dynamics; the equations are the same and the ideas are the same, only the acceleration, in statics, is zero.

(4) *Graphical methods confuse learners of statics.* Here I rely on experience, and report what I have observed. I have noticed, over and over again, that, while a learner of analytical statics may fail to solve a problem, he yet knows what he is trying to do, and he does not, as a rule, lose sight of principles.

But I find that beginners, who have learned something of graphic statics, appear to lose sight of principles altogether, and are content to make the wildest " shots." They make triangles out of ladders, walls, and ground, and continually take the lengths of bars or strings to represent the stresses in them in their attempts to " get a triangle of forces."

I find no beginner so difficult to teach as one who has learned some graphic statics at a preparatory school, and I much prefer those who have learned no statics at all. There seems to be something in graphical methods that paralyses the learner's powers of thought and reasoning, or at least allows them to slumber.

To sum up. I have come to the conclusion that graphical methods (as defined above) should be reserved for a relatively late stage in any educational course in mechanics, or even be omitted altogether until required for special work. In addition to the reasons given above, I may add that graphical work consumes an amount of time that seems out of proportion to the mental training and knowledge of principles gained. W. LARDEN.

Devonport, April.

Sunspots and Temperature.

THE following view of this subject (related to that given by Dr. Lockyer a short time ago) may be of interest.

Consider the last five sun-spot waves, as measured from the first year after a minimum to the next minimum, thus :—

(1) 1844–56 (13 years)	...	...	Maximum	1848
(2) 1857–67 (11 years)	...	...	,,	1860
(3) 1868–78 (11 years)	...	...	,,	1870
(4) 1879–89 (11 years)	...	...	,,	1883
(5) 1890–1901 (12 years)	...	...	,,	1893

Using Wolf and Wolfer's sun-spot numbers, and finding the annual average for each of these waves, we get the curve marked A (dotted line) in Fig. 1 (p. 608).

Ascertaining next the averages of several meteorological items at Greenwich for those periods, we obtain curves B, C, D, E, F. The items are :—

(B) Mean temperature of winter (December–February).

(C) Frost days in winter (an inverted curve).

(D) Days with maximum temperature 70° or more (in year).

(E) Mean temperature of summer (June–August).

(F) Mean temperature of year.

The amount of agreement between these weather curves and the sun-spot curve seems remarkable. The sun-spot wave with *highest* average number (that for 1868–78) corresponds with the time of *greatest* warmth in each case, and